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MUST HISTORIANS REGRESS? AN ANSWER TO LEE BENSON

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ABSTRACT

In a recent symposium published in Historical Methods, Lee Benson, once a forceful proponent of more systematic methods and models in history, announced that it had all been a mistake. Applied to human affairs, Occam's Razor merely results in useless carnage. Wo/men are too complex and contradictory to fit into any simple theories, much less laws, and any attempt to formulate or test any such statements merely misleeds. To redeem social science and save the world, we should abandon the attempt to postulate abstract, value-free, and often mathematicized hypotheses and to validate them through the use of sophisticated statistical techniques. Instead, we should return to a Marxism cleansed of the notion of class conflict, and to a combination of easy arithmetic methods that everyone can understand and careful analysis of qualitative data.

In his 1961 Concept of Jacksonian Democracy, Benson now claims, he was wrong to adopt the idea of voting cycles from economics, because the polity is more complicated than the economy, and to pretend that he had arrived at his conclusions on the basis of "hard" data, when he had used a mixture of qualitative and quantitative information. That historians have been wrong to adopt regression analysis to relate aggregate voting to socioeconomic indices he attempts to demonstrate by commenting on one table from my Shaping of Southern Politics.

None of Benson's contentions except his mea culpa will stand up to rigorous examination. His conception of science is distorted, the economy is more complex than he imagines, and an extensive analysis of his scattershot critique of my table and of regression analysis reveals that he is wrong in every particular. His charges of scholarly irresponsibility collapse when set against his own practice. Indeed, it seems possible that he found no evidence of a class basis for political divisions in Concept because he used a poor measure of it and performed no systematic multivariate tests.

Benson's general anti-scientific stance would force historians and social scientists to abjure the use of powerful methods and theories, would make generalizations and the replication of results impossible, and would cause history needlessly to regress to a pre-scientific stage. The fallacy has not been in our mistransference of scientific modes of thought and analysis, but in his own misunderstanding, misconceptions, and mistaken arguments.

MUST HISTORIANS REGRESS? AN ANSWER TO LEE BENSON

In a series of books and articles published from 1957 to 1961, Lee Benson launched an attack on previous political historians' implicit theorizing, faulty inferences, and failure to examine relevant data using multivariate methods.[1] Castigating as "fiction" the view of Jacksonian Democracy embodied in Arthur Schlesinger Jr.'s Age of Jackson, for example, Benson announced that he had rejected it because he had "penetrated the rhetorical surface and struck hard data."[2] Distinguishing between factual and interpretative questions, he sought to reduce the scope of "subjective relativism" among historians by first "objectively reconstructing" the facts, thus putting historians in a far better position to pose interpretative questions "in meaningful and reasonably precise form."[3] To attain objectivity, historians had first to discard "the impressionistic approach long dominant in American political historiography," and adopt a "systematic methodology," one that required data to be analyzed "comprehensively and rigorously."[4] The young Lee Benson's vision inspired many historians, myself included, to attempt to carry out a new, more thorough and "scientific" program to revise American political history.

Having helped to lead the march up the hill of social scientific history a generation ago, Benson now proposes to march back down again and rejoin the historians who never stirred from the valley of

tradition. In "The Mistransference Fallacy in Explanations of Human Behavior," he assaults "the quality of social scientific explanation in general" because other social scientists do not share his current a priori assumption that human behavior is "variable, discontinuous, contextual, ephemeral, and complex." Cartesian reductionism may be appropriate to "the simple and stable phenomena studied by physical and biological scientists," but "Complexity, not simplicity, is the human condition. Methods that require and/or exalt parsimony tend strongly to caricature social reality."[5]

Hard science and social science, Benson now asserts, should develop differently. The classical (and fictitious) model of science--postulate a theory, draw out its implications logically, test it through objective experiments, reformulate the theory if necessary, and repeat--is not applicable to the study of humans. Students of wo/man's behavior should, like Marx, start from an explicitly value-laden position, abandoning the notion that inquiry can be value free, proceed to study "practice," and only then work out theories relevant to the practical problems involved in attaining their desired goals.[6] Neoclassical economic theory has been too abstract and mathematical, and has ignored qualitative changes; econometrics is based on assumptions that the dodgy data will not bear; "surveys are frequently found to be grossly erroneous."[7] Historians should therefore not mimic the fallible economists, political scientists, and sociologists. More specifically, they should employ only "appropriately simple statistical techniques (e.g., changes in arithmetic percentages)." In fact, "The

more esoteric and complex the statistical techniques used in historical research, the more worthy of skepticism and suspicion the researcher's claims."[8] Since accepting Benson's critique, or what he modestly terms "my Plan of Redemption for American social science" would require abandoning much of current social science history, as well as most of the other social sciences, it is worth examining in some detail.[9]

Benson's blithe generalizations about the need to avoid generalization and his blanket condemnations of whole disciplines, rather than of specific examples of social scientific work, are not susceptible to argument. There is no way to shake a firm, ultimate belief that the trees never form a forest, or that they always do, and it is only by focusing on particular studies that we can productively discuss whether the necessary simplifications of a theory or model are too gross a distortion of reality to yield interesting insights. Benson therefore turns to two concrete cases, gently chiding his own Concept of Jacksonian Democracy and rather more harshly condemning my earlier work, the methods that I used, and the conclusions that I reached. Do his specific examples offer any support for his broader critique of social science?

As his first instance of mistransference, Benson discusses his application of the concept of economic cycles to electoral behavior, and what he claims was its further development by Walter Dean Burnham into critical realignment theory.[10] States and counties, not the nation as a whole, are "the real aggregate units on which the American electoral and party systems are actually based," he asserts. His

mistake in Concept, he now believes, was to apply to "the extraordinarily complex federal [political] system" generalizations about economic cycles drawn from analyses of what "can reasonably be viewed as a national economic system."[11]

There are two problems with this reasoning. First, what the appropriate level of aggregation is depends on the theory being tested, the questions asked, and the data available. The issue cannot be decided in advance, by assumption, for every conceivable subject in American political history. Theories about individual political or economic behavior require data on individuals or inferences from aggregate data to individuals. Townships, counties, states, and nations make no choices--individuals do. Hypotheses based on individual social choice or on the reactions of individuals to positive or negative reference groups, for example, must therefore be founded on the observation or estimates of the behavior of separate persons, not on crude guesses from inappropriately aggregated data, as Benson does in his arithmetic manipulations of county election returns in his work in progress with Joel Silbey. Furthermore, whether one focuses on variations in action within townships, counties, states, or the nation, or between nations depends on what topics one is concerned with. There is nothing wrong in principle with discussing national voting patterns. It may be instructive, for instance, to ask whether serious third-party movements have tended to arise during economic crises or whether war has helped or hurt the reelection chances of incumbent presidents. It happens that in the United States, because of the decisions of the

Census Bureau and of compilers of election results, much of the most interesting data is available on the county, rather than the township or individual level, but the arbitrary decisions of imperfect humans should not be raised to the status of laws. There is simply no right answer to the question of what the proper level of aggregation is.

Second, the American economy is at least as complex and variant as the polity, and the business cycle theorists whom Benson cites in Concept did not ignore variations within the national economy.[12] At most times in most places in United States history, voters have been faced, in partisan elections, with a choice between candidates of the same two parties wherever they lived. Consumers of food, clothing, shelter, and every other type of good, on the other hand, have been offered nearly innumerable choices, especially since the economy became a national one in the late nineteenth century. Before that time, the alternatives facing individual consumers were more restricted, but they varied more widely from locality to locality.

The cycles of production and investment varied enormously from region to region, state to state, and county to county during the 19th century. In textile production, the first large industry in the country, for instance, annual changes in output in Rhode Island from 1826 to 1860 had a bivariate Pearson product-moment correlation coefficient of just 0.138 with those in neighboring Massachusetts, and 0.000 with those in Maine and New Hampshire; while a similar correlation for Massachusetts versus its northern neighbors was 0.548--more respectable, but far from perfect. Regional variations in

railroad investment in the last four antebellum decades, which should have been damped down by international movements in capital and international trade in rails, were similarly high, as the correlation matrix of annual changes in Table 1 shows. In short, the antebellum economy was by no means an uncomplicated national whole, and if Benson transferred a simplistic theory from economics to politics, the fault was not in the analogy, but in what economists would refer to as his "stylized facts."[13]

(Insert Table 1 Here)

Even today, the sun belt/rust belt, or high tech/low tech/no tech, or gentrifying/decaying categories are merely convenient tags that express basic trends, but shroud vast discrepancies within regions, states, cities, and even census blocs. While we have few figures on basic economic trends aggregated at the state level and collected often enough to give us a numerous time series before the post-World War II era, the estimates of personal income made quarterly by the Commerce Department from 1948 to 1982 show that trends vary somewhat from state to state. For instance, the bivariate Pearsonian correlation coefficient between the quarterly changes in income for the United States as a whole and those in New York state is 0.745; for the United States and Tennessee, 0.828; for the United States and North Dakota, only 0.363.[14]

Business cycle theorists such as Alvin H. Hansen, whom Benson cites as his only source for cycle theory, explicitly recognized the

presence of divergent economic cycles. Hansen devoted his third chapter to "the building cycle," which he declared was "much longer than that of the major business cycle." He identified four building cycles and seven "major cycles" from the 1870s to the 1930s, and also noted a two year cycle in textile production and a three to four year cycle in hog production. Changes in consumption and investment, Hansen found, were by no means perfectly correlated between 1921 and 1931, and in a chapter on "Econometrics in Business Cycle Analysis" in Hansen's book, Richard M. Goodwin demonstrated, for instance, that iron and steel consumption in the United Kingdom from 1920 to 1936 did not closely follow trends in the economy as a whole.[15]

That critical realignment or voting cycle theory has problems I do not deny.[16] That contrary trends on various levels of the polity is one of those problems, given current formulations of realignment theory, I agree. But that those difficulties were the result of a bad analogy between an uncomplicated economic system and a complex political system, or that economic cycle theorists represented the economy as unitary is based on misunderstanding and misreading. The flaws in voting cycle theory are not the result of mistransference.

Turning from theory to methodology, Benson launches a barrage of arguments against political historians who use the statistical technique of regression analysis on data aggregated by wards, townships, counties, etc., whom he terms, in a frightening-sounding and

curious neologism, "ecological regressionists." First, he maintains that the approach requires two unjustifiable assumptions--linearity throughout the larger unit being studied, such as the state, and the irrelevance of other independent variables that were not included in the equations. Ignoring "contextual factors" is "preposterous," he charges, because "All [American] politics is local" and because the interaction between group membership and locational characteristics varies widely across times and places. His argument and his earlier work implies that he believes that other, simpler methods, particularly that of focusing on areas that are "homogeneous" with respect to one variable, such as heavily "puritan" townships, require no such assumptions. Second, he criticizes those who employ regression analysis because "they must assume that they know the answers in advance." With no apparent ironic intention, he immediately proceeds to condemn "regressionists" on the third ground that they treat mere "statistical aggregates (e.g., income strata) and members of real social groups (e.g., Irish Catholics, German Catholics, evangelical Yankee Presbyterians, Old School Presbyterians)" as equally useful categories. Fourth, "ecological regressionists" falsely claim to be employing multivariate models when they in fact only use bivariate ones. Fifth, their "highly esoteric" methodology is so "unintelligible to the vast majority of historians" that the only way to convince them of the validity of these charges is to focus on a substantive instance. His example is one of the 66 tables in my Shaping of Southern Politics.[17]

It is useful for the purpose of clarification to translate Benson's points into explicit equations. The usual regression equation is:

$$1) \quad Y = B_0 + \sum B_i X_i + u,$$

where Y is the dependent variable, B_0 is the intercept, the B_i's are the slopes or coefficients relating to the independent variables, the X_i's are the independent variables, u is an error term, and the subscripts denote variables. (Subscripts for individuals and for aggregate units have been suppressed for the sake of simplicity of presentation.)

If the X's are all simple interval-level variables measured on aggregate units, such as the percentage black or the percentage Catholic, and if the parameters, the B's, are estimated using ordinary least squares (OLS), then the relationships modeled are linear, and the parameter estimates will be unbiased if and only if the "expected value" of the error term u is zero. This latter condition will not hold if the "true" model, i.e., the way the world really works, is:

$$2) \quad Y = B_0 + \sum B_i X_i + \sum C_j Z_j + v,$$

where the Z_j's are additional independent variables that should have been entered into the equation, but for some reason were not, the C_j's are their associated coefficients, and v is an error term which differs from u.[18]

The fallacies that Benson asserts are inherent in ecological regression may be stated as follows:

F1) relationships that are in fact nonlinear cannot be estimated accurately (the linearity point);

F2) the X's cannot involve any terms expressing the possible interactions among independent variables, and, in fact, all equations must be bivariate (the bivariate/multivariate point);

F3) the C_i's = 0 (the excluded variables point);

F4) the B's are "small" and the error term is "large" (the "politics is local" point);

F5) substantively, some of the X's describe the actual world, while others are merely artifacts of the available collections of data;

F6) all this is gibberish to most historians.

Benson's charges are based on misconceptions, patent misunderstandings, unjustified <u>a priori</u> assertions, and hyperbole.[19] The last three points may be dealt with quickly and simply. Sometimes politics may appear local--that is, the variations in behavior across aggregate units may not be adequately explained by the available indices of their socioeconomic composition--but often it does not. Surely this is a question to be decided on the basis of evidence in each instance, not one to be settled for the entire course of American

political history by casual allusion to a catch-phrase. Nor does Benson offer any empirical basis here, or, as far as I know, elsewhere in his work, for his Platonic proclamation about the reality or unreality of ethnocultural and class divisions.[20] Being less committed to a predetermined stance than Benson is, I believe that the political relevance of ethnicity, religion, race, class, economic interest group, section, etc. varies from place to place and from time to time (though not randomly), and that the weight that should be given to each in a specific case is a matter of evidence and inference, not dogma.

If most historians are as ignorant of statistical knowledge and as incapable of understanding regression analysis as Benson asserts, it is a state to be altered by study, teaching, and writing, not one to be encouraged by summarily dismissing all attempts to proceed beyond grade-school arithmetic. Moreover, substantial numbers of historians have taken the beginning methods courses now offered around the country or at one of the summer institutes, or have read through an elementary text; mainstream historical journals with increasing frequency publish articles that make use of statistics; more specialized journals such as Historical Methods, Social Science History, the Journal of Interdisciplinary History, Explorations in Economic History, and the Journal of Economic History are brim-full of numbers and equations; and clear and simple introductions to regression abound.[21] The history profession need not contain, and, in fact, no longer does contain as high a proportion of statistical illiterates as Benson avers, and his attempt to scare off those who are still timorous or to confirm them in

their comfortable rationalization for continued ignorance only impedes progress in the profession.

To return to Benson's more important points: As the very table that he concentrates on demonstrates, one need not assume, as he claims, that "all the individuals classified as belonging to a specified group essentially vote in the same way (including nonvoting) in all the political entities of a state." In my original article on "ecological regression," I emphasized the desirability of examining scatterplots carefully to check for nonlinearities and deviant counties, and I illustrated that advice by estimating black and white voting by regions in Tennessee, by separating counties above and below 30% black in North Carolina, and by introducing a quadratic (squared) term in a set of regressions for North Carolina.[22] In a 1980 paper, I explained how one could use nonlinear logit techniques and weighted as well as unweighted OLS in the analysis of data. In other papers and books published long before Benson's "Mistransference" article, Allan Lichtman, Laura Irwin Langbein, and I have shown that it is possible to employ quite complex multivariate ecological regression models containing interaction terms and a wide variety of independent variables. Furthermore, we demonstrated that simpler methods, such as presenting data from more or less homogeneous areas share all the assumptions criticized in F1)-F4), but that since they allow no possible tests of those assumptions, the elementary methods are inferior to more sophisticated regression techniques.[23] Since the

linearity assumption can be relaxed, Benson's first two arguments are simply wrong.

F3) is potentially more serious. Any formal or informal estimation technique must of course be based on the belief that measured or unmeasured variables that are excluded from the equation do not bias the estimate of relationships among the included variables. To put it more simply, incorrect models give incorrect estimates. Now, it is obviously true that in social scientific and perhaps particularly in historical problems, we will never have data on all possible variables that influence both the dependent and the included independent variables. But note that if an excluded variable or set of variables has a nonzero correlation with the dependent, but not the included independent variables, or with the included independent, but not the dependent variable, then the C's will be zero, and the B's will be unbiased.

Consider an example of particular interest to political historians, the estimation of "transition probabilities" between those who voted for each candidate in one election and their choices in a subsequent election. Suppose that a set of characteristics--economic, ethnocultural, or whatever--causes people to vote a certain way in the first election, but that these variables have no influence in the second election that is additional to or different from what they had in the first election. The relation between socioeconomic attributes and political choice, in other words, is roughly constant in both elections. This will be the case, for instance, if the "ethnocultural

thesis" is approximately correct: that is, if ethnic and religious groupings fundamentally underlay political alignments over a series of elections. In this event, if the X's are the percentages for each party, the Z's are the demographic traits that caused the first political alignment, and those who enter and exit the electorate are either ignored or explicitly taken into account, then the C's will be zero, and it will not matter whether equation 1) or equation 2) is estimated. Even if we do not include the Z's in the equation for the first election, either because we neglect to do so, or because we have insufficient data, so long as the C's would actually have been zero in the equation for the second election, the B's will be unbiased.

A second case is that in which the C's are not precisely zero, but they are small. Then the B's may be slightly, but not very biased, even if we incorrectly use equation 1), instead of equation 2). For substantive purposes, it will not matter much which equation we use in this situation, either. The only circumstance when an interpretation will depend crucially on whether equation 1) or 2) is used is that in which the C's are large, and equation 2) is mistakenly employed.

Since there is no way to determine in general the frequency of the three classes of cases, let us turn to Benson's example. Was my division of the state "arbitrary," my picture of voting created with an intention to "fool the eye and boggle the mind" in a way that "caricatures multivariate analysis and systematic, responsible research," as Benson's indictment reads? Were my parameter estimates of black and white voting for the Democratic and Republican parties in

the 1880 and 1884 elections in Tennessee severely biased because I left out independent variables that I ought to have included?[24]

Examination of the scatterplots relating the percentage black in each county to the proportions of adult males voting Republican and Democratic suggested that running regressions on subsets of the counties might improve my estimates of the voting patterns by race.[25] The splits in two secession referenda, as well as comments in books and newspapers published during the Reconstruction and post-Reconstruction eras, and the folk knowledge acquired from growing up in the state made a sectional breakdown an obvious candidate for the sub-grouping.[26] But how should the state be divided--into the traditional three sections, or into some combination of the two separated from the third? Approaching this question empirically, back in the antediluvian days of 1968, which all my data resided on punched cards, mainframe batch processing was the only mode available to me, and when I did not understand how to run the regressions more easily, using a dummy variable approach, I simply physically divided the cards into three (later, two) bunches, according to the traditional division of the counties into sections, and ran separate regressions.[27] Since it is much easier to explain how to choose between different models using dummy variables than running regressions on subgroups of the counties, and since other historians may profit from an explicit statement about how to test for regional effects, it will be useful to frame the discussion in those terms.[28]

If the relation between, say, race and voting for the Republican candidate in a certain election were linear across all the county units in a state, one could estimate it as:

$$3) \quad R = B_0 + B_1 N + u,$$

where R is the percentage of adult males who voted Republican in each county, N, the percentage of adult males who were black, the B's the relevant coefficients, and u the error term. In this simple equation, B_0, the intercept, could be interpreted substantively as the Republican percentage in a hypothetical average county containing no blacks at all (i.e., where N = 0), and B_1 would express the difference between the rates at which blacks and whites voted Republican. In an imaginary county that was 100% black, N would equal 1.0, and the percentage of votes that the Republicans would expect would therefore be $B_0 + B_1$. Similar equations could be estimated using the Democratic and nonvoting percentages as dependent variables.

If one had reason to believe that the relationships between race and party were similar in, say, two sections of a state, but that people living in one region had a generally higher propensity to vote Republican--because of historical experience, custom, a regional candidate, or whatever--then a regional "dummy" variable could be added:

$$4) \quad R = B_0 + B_1 N + B_2 E + u,$$

where E takes on the value one if the county is in a particular region,

such as East Tennessee, and zero if it is not. Two tests would determine whether, in a particular case, 4) yielded better estimates than 3): Is B_2 statistically significant, using the standard "t" test, and is the percentage of variance explained by 4), adjusted for the diminished "degrees of freedom," higher than that explained by 3)?

It might be that the denizens of each section differed not only in their general inclinations toward the parties, but also in how whites reacted to blacks and vice-versa. Suppose that in one section, there were too few blacks for whites to be very concerned with, and that the whites' behavior therefore did not vary much with the proportion black in each county. In the other section, where the percentage of blacks was in general larger and differed widely from county to county, however, whites might have been much more sensitive to increases in black density, and the relation between race and politics would have been closer. (These "stories" are only meant to be illustrative. A wide variety of mechanisms could underlie regional behavior differences.) To investigate this hypothesis, one might estimate:

$$5) \quad R = B_0 + B_1 N + B_2 E + B_3 E\,N + u,$$

where E N is the percentage black in the counties of region E (because it is 1 times N there), and zero otherwise. Substantively, B_0 is the estimate of the proportion of whites who voted Republican in the other region, which may be referred to as W; B_1 is the difference between black and white rates of Republicanism in region W; B_2 expresses the greater or lesser propensity of easterners to favor the Republicans;

and B_3 captures the difference in the relationships between race and voting patterns in the two regions. The same tests described above would determine whether to use 3), 4), or 5) for estimation purposes. Different combinations or definitions of regions could be tested against each other. Separate regressions for each of two regions are equivalent to equation 5).

Table 2 gives the results of estimates of various models of voting by race in the 1884 gubernatorial election in Tennessee.[29] The three panels of the table contain OLS regression coefficients for equations predicting the Democratic and Republican votes and the percentage of nonvoters, respectively. The statewide estimating equations, numbered 1 in each panel, do not fit very well. Only in the nonvoting equation is the coefficient for the percentage black significant at the conventional 0.05 level (which corresponds to a "t" value of 2.0 or above), and even there, the independent variable explains only 6% of the variance in the dependent variable. Model 2, containing a term for the percentage black squared, represents an improvement. Heavily black counties recorded higher percentages of Republican votes, and had somewhat lower proportions of voters overall than did heavily white counties. Equations 3 and 4 show that West Tennessee did not deviate very markedly from the statewide trends. The regional coefficients differ significantly from zero in only two of the six cases, and the R^2s are generally low.

(Insert Table 2 Here)

Equations 5 and 6 of Table 2 yield better fits, particularly for the Republicans. The GOP was, at this time, a somewhat uneasy coalition between upland whites in the east and lowland blacks in the west, and a model that separates out the middle section therefore performs more adequately than the previous ones. But equations 7 and 8, which split off East Tennessee from the other two sections, work best of all the regional hypotheses. Every coefficient except one differs statistically from zero for the Republicans and Democrats, the regional dummies are significant for nonvoting, and in every case, equations 7 and 8 explain more variance than any other models so far considered.

It is difficult, however, to choose between these two hypotheses. Only in the Republican equation is the term for a regional difference in slopes (% black times E) significant at the 0.05 level, and the additional percentages of variance that Equations 8 explain are quite small. Subatantively, the reason for this is that there is little variation in the percentage of blacks across the counties of the eastern section, and therefore the estimate of the slope there is not very reliable.[30] Only 14.3% of the state's blacks, but 32.93% of the whites lived in East Tennessee. Table 3 was calculated by multiplying the regional estimates of the racial voting percentages by the proportion of each race who lived in the section, and then summing the results. It shows that equations 7 and 8 from table 2 produce quite similar overall results. One might well choose equation 7 on grounds of simplicity. More generally, the discussion of table 2 underlines

the point that "ecological regressionists" need not be tied to a single model of voting, but that, on the contrary, it is easy to compare the results obtained from a range of different models and to choose the model or models whose assumptions best satisfy explicit numerical criteria.

(Insert Table 3 Here)

Benson suggests that four types of variables that I did not include in my regressions for voting by race may have systematically biased my conclusions: religious, ethnic, "urban, rural, farm," and economic. Temporarily abandoning his distaste for generalizations about human behavior, he announces his belief that differences between Methodists, Baptists, and Presbyterians, or between voters of Scotch-Irish and those of English descent are--apparently always--"theoretically significant," and that "any serious work on the social bases of American voting behavior" cannot ignore them.[31] Presumably, in Benson's mind, the absence of political issues that could plausibly have divided these groups from each other, and contemporaries' failure to mention any divergent patterns among white ethnic or religious voters do not count as evidence against such a hunch. The fact that post-Reconstruction Tennesseans overwhelmingly concentrated on the political divisions between whites and blacks, and, among whites, on regional differences, and that they never, so far as I know, referred to ethnoreligious differences among whites in discussions of the state's politics, is consequentially irrelevant. In the world

according to Benson, certain social formations (but not economic ones) seemingly always define other social formations as "negative reference groups," and politics is merely the expression of these inevitable tensions.

At the same time that he insists on their importance, Benson pours scorn on attempts to measure particular variables. Because churches set different qualifications for membership, religious statistics "are notoriously unreliable." Since they include people ineligible to vote because of age or sex, and since not every voter is a church member, they are inaccurate indices of differences in the religious identities of voters from county to county. To use such statistics, Benson pronounces, "violates elementary rules of evidence and inference."[32] As will be noted below, he makes similar criticisms of the use of available economic statistics.

If Benson believes in general what his comments on specific variables imply, that is, that any variable that cannot be perfectly operationalized should not be employed, then his position would make all historical research and most social scientific research impossible. No social historian ever has a flawless index of class or ethnicity or culture; no intellectual historian ever has a complete picture of his or her subjects' thought; no economic or demographic time series is ever entirely unproblematic; no political historian can ever be certain that the votes s/he analyzes were absolutely freely cast, fairly counted, or errorlessly recorded. To insist that historians use only impeccable and complete data is to deny them all research material,

"literary" as well as numerical. In the particular case, moreover, it is doubtful whether the membership practices of the major Protestant denominations that dominated Tennessee differed significantly. There is also no reason to believe that the sex composition of these churches varied markedly, or that nonmembers' religious preferences differed appreciably from those of their neighbors--that, for instance, in a predominantly Methodist and Baptist county, non-communicant voters were secret Catholics or free thinkers. (The idea of an "atheist vote" in Tennessee is, to use one of Benson's favorite terms, "preposterous.") In any case, statistics on communicants from the 1890 United States Religious Census are the only figures available on the county level, and unless others can propose a better index, it is necessary either to use them or to give up entirely any attempt to measure the impact of religion on Tennessee politics.

Table 4 presents statewide percentages and county-level standard deviations of ethnic, religious, demographic, and occupational variables for Tennessee in 1890. Ethnically, 96% of the whites in Tennessee in 1890 were at least third generation Americans, virtually all of whom shared a common language. It is extremely difficult to see how tight ethnic communities could have been maintained, and, in any case, it is impossible to separate "Scotch-Irish" and "English" who had immigrated to America in the far distant past. Religiously, 88% of the whites who belonged to a church were members of four Protestant denominations--Methodist, Baptist, Presbyterian, and Campbellite--all of them fundamentalist, nearly all Arminian in theology, and none

dissenting from the southern white consensus on social issues: it was wrong for the federal government to intervene in race relations, but proper for the state to establish racial segregation and, most agreed, to ban alcohol. On first appearance, the religious tenor of the state seems to offer little room for conflicts between "pietists" and the tiny bodies of "liturgicals."[33] Only 14% of the state's population was located in places containing over 2500 people, and nearly two-thirds of the persons employed, white or black, were farmers.[34] Even among white males over ten years old, only one in nine was employed in mining or manufacturing. In sum, the state's whites were socially very homogeneous--native-born, fundamentalist, rural farmers. The only major ethnic, religious, or demographic dividing line was racial.

(Insert Table 4 Here)

Table 5 adds to Equations 8 of Table 2 variables representing the percentages of white church members of the various denominations, as well as the percentages of the whole population who lived in "urban" areas and those who were born abroad.[35] Only 4 of the 30 new coefficients in Equations 2 are significant at the 0.05 level--the Baptist and Lutheran parameters for the Democrats, the Lutheran coefficient for the Republicans, and the foreign-born estimate for nonvoting. Examination of the scatterplots for the Lutheran and foreign-born variables indicates that they ought not to be taken seriously. The three counties that were over five percent Lutheran happened to be rather strongly Democratic, but the 70% Democratic vote

in one of them can hardly be attributed to the eight percent of its white church members who were Lutherans. Likewise, there were only four counties that were more than three percent foreign-born, and all of them had below average turnout. But the fact that 43% of the potential electors in one county did not vote cannot have been caused by the presence of a group of the foreign-born amounting to only five percent of its total population. Indicators that do not vary much and whose regression relationships are produced by a small number of extreme cases should be disregarded.

(Insert Table 5 Here)

While it is true that the proportions of variance explained rise when additional variables are added--those adjusted for the addition of further variables increase by 16% for the Democrats, 5% for the Republicans, and 6% for nonvoting--it will almost always be the case that shoving more column vectors into a regression equation will increase its predictive power. To demonstrate this, I fed six sets of computer-generated random numbers between zero and one into regression equations that already contained the variables from Equations 8 in Table 2. The results in Table 6 demonstrate that even with these "garbage" independent variables, the adjusted proportions of variance explained increased in two of the three cases, the coefficients already in the equations shifted somewhat (compare the first four coefficients in Table 6 with those in Equations 1 and 2 of Table 5), and one of the eighteen new variables was statistically significant at the 0.05

level.[36] It is hardly surprising, therefore, to find that in table 5, one of the variables, the Baptist percentage, has a statistically significant relation with one of the dependent variables that cannot be accounted for by a few extreme cases; that the R^2's rise, or that there are small changes in the coefficients for variables previously entered into the equation. The results of the equation containing fictitious variables also points to the desirability of pruning equations of variables whose coefficients may well have been produced by chance. When statistically irrelevant religious and demographic variables are eliminated, Equations 3 of Table 5 reveal that the Baptist percentages have a fairly marked effect on the Democratic and Republican percentages, although not on nonvoting.

(Insert Table 6 Here)

What substantive significance can be attached to the fact that Baptists appear to have favored the Republicans, and that the effects of regional, but not racial variables are reduced when the proportion of whites who were Baptists is taken into account? The short answer is: not much. White Baptists tended to live in East Tennessee, which was 45% Baptist, while Middle and West Tennessee were only 23% Baptist. Furthermore, Baptists during this era were rather delassein Tennessee. An equation containing a dummy variable for East Tennessee and a measure of white wealth per adult male explains 33% of the variance in the percentage Baptist in 1890. Consequently, the "Baptist effect" may well be more a measure of regional history and the class attitudes of

the church members than a consequence of their sectarian outlook. In any case, adding the political influence of Baptist membership to estimates of the statewide totals of whites and blacks preferring the Democrats or the Republicans, or not voting makes little difference (compare Tables 3 and 7). In a county containing the mean percentage of white Baptists, the summed effects of this and of the other variables in Equations 3 of Table 5 produce estimates of statewide voting by race very similar to those in the table that Benson attempted to discredit; My conclusions about racial voting differences would, if anything, have been slightly strengthened had I included the Baptist percentage in my equations.

(Insert Table 7 Here)

To prove a negative conscientiously takes much more time, space, and detail than to make blanket, totally undocumented assertions, as Benson did, about the potential effects of numerous variables and estimating procedures. By now, however, it should be clear to the reader that I arrived at my estimates with some care, that nothing inherent in the methodology prevents tests of a variety of multivariate models, that my conclusions were not significantly affected by variables excluded from my equations, and that all six of Benson's criticisms must be rejected either generally or at least in the case of the example that he attacks.

Benson ratchets up his level of rhetoric to a shrillness that is fortunately seldom matched in recent historical controversy in considering my attempt to relate class position to political choice among whites in Tennessee. My failure to discuss explicitly my conception of class and my employment of the only available indicator of class in Tennessee, assessed real and personal property value, which other historians have used as well, convicts me of scholarly irresponsibility, he insists. Indeed, "to characterize [Kousser's] work as irresponsible seems to take refuge in delicate understatement." Thereafter eschewing understatement, he labels my discussions of class, "cloudy and confused," and my decision on what variable to employ as an indicator of class, "mind-boggling...astonishing...a truly breathtaking kind of historical malpractice." American tax assessments, he asserts, "are notoriously unreliable, erroneous, misleading, deceptive, and fraudulent"; they mix property belonging to businesses with that held by individuals; and county-level averages hide variations within those units. To employ such "terribly unreliable and erroneous" statistics as an indicator of class demonstrates "how grossly [Kousser] violated the norms of responsible acholarship."[37]

This abuse seems rather uncharitable to a fellow scholar, coming as it does from one whose discussion in his chapter on "Class Voting in New York" in Concept is hardly clearer than my own, much briefer treatment in Shaping, and who used a directly analogous and arguably inferior wealth-related index of class in his own work. To be sure, even though I generally employed wealth as the mark of class

differences, I once spoke of income, and never of occupation. In an overwhelmingly agricultural society, income and wealth are closely related, and, lacking occupational statistics below the state level, I could only have speculated, which I chose not to do, about the relatively small effects of the votes of professionals, miners, or manufacturing employees. (See Table 4.) In Benson's own chapter, he referred to "workers" (p.141); the "wealthy," the "poor working-class," and the "mechanics" (p.144); the "well-to-do" (p.145, n. 33); to "wealth" and "occupational groups" (p.148); to "prosperity" (p.148); to "hard-pressed tenant or debtor-farmers and great landlords or land companies" (pp.150-51); and to "prosperous farmers" and "poorer farmers" (pp.155-56). By occupational groups, he appeared to refer to what others would call interest groups: wool growers (p.158), bankers (p.159), import and export merchants (p.161), and manufacturers (p.161). He never straightforwardly defined what he meant by "class" in the chapter, and never attempted to discover the political proclivities of groups that other scholars of New York politics have concentrated attention on, for example, skilled and unskilled workers.[38] The point of this recital is not merely tu quoque, but rather that the topic of the political importance of class in America is too complex, and the treatments of it, even by the most careful commentators, too fuzzy for any student of the subject to begin casually tossing boulders at another.

For his analysis of the 1844 election in Concept, Benson classified rural towns in New York state into five categories on the

basis of data on the value of dwellings per family from the 1855 and 1865 New York State Censuses, several gazetteers published from 1824 to 1872, and twentieth century studies of New York land values.[39] He never specified the weight given to each of these chronologically and methodologically diverse sources, never attempted to justify the number of categories (why not three? why not seven? why not use the raw, interval-level data?) or the boundaries between them (why $300 instead of $250 or $350 for the dividing line between "poor" and "marginal"?), never tried to determine what difference changes in his arbitrary definitions would make, never disclosed what proportion of towns fell into each category, and never presented data on the relationships between class and voting for all the towns in more than one county.[40]

To determine the similarity of Benson's index and my own, I regressed his index, which I computed using county-level data in the 1865 New York State Census (the only one of his sources available to me at the time) on the real and personal property value per adult male citizen from the 1870 United States Census. The bivariate correlation coefficient was 0.755. Because Benson asserted without explanation that dwelling values for urban areas "could not be accurately constructed from the available data, and did not constitute a valid measure of wealth," I also ran the regression leaving out the four most urban counties (Albany, Erie, Kings, and New York), which contained 84.5% of the state's population living in cities that were divided into wards.[41] Because deleting them reduced variations in wealth, the correlation dropped to 0.555. Eliminating all cities that contained

wards from the State Census data diminished the correlation to 0.501.[42] Since Benson might argue that data aggregated at the county level was unrepresentative, I sampled one town or ward randomly within each county and then regressed that unit's dwelling value per family on the county-level dwelling value per family, again excluding the four most urban counties.[43] The correlation coefficient was 0.721.

It is not clear on principle which of these rather closely related indices is better, even if all of the problems which flow from Benson's decision somehow to combine disparate data collected over a century are ignored.[44] Benson did not appear, in any statistics that he used explicitly, to take land values into account, and he ignored wealth invested in businesses. This amounted to assuming that every person decided to invest the same proportion of his or her capital in housing, as opposed to alternative investments in land, business, and personal property, which is surely incorrect. Excluding land and business property and that in cities, of course, reduced both the mean and the variance of the wealth distribution. If one were designing a measure to minimize the potential effect of wealth on politics, and one of Benson's major themes, of course, is that there was little relation between the two, then one would choose his index.[45] And while his town-level statistics do have the advantage that they capture within-county variations in wealth, they have the drawback of requiring that one posit that all property within a town was owned by men who reside there.

The federal-based statistics, on the other hand, do take into account the values of land, business, and personal property, but they necessarily ignore differences within counties. Personal property is difficult to measure and easy to hide. And businesses, especially railroads, may be owned by persons outside the county. Both indicators, of course, derive from someone's assessments, either those of tax collection officials or ones made by individuals appraising the value of their own dwellings or personal property. Both sets of figures share the inexactness and incentives for misreporting of all such data. Unless it is possible to correct the data for over- and under-assessments, then the only alternative to using one or the other of the indicea is to make no systematic analysis of the relation between wealth and voting at all. Allowing free rein to speculation, this option also has two other qualities that may appeal to some: it requires little work, and its conclusions cannot be refuted. Whatever Benson may think about these attributes of unsystematic methods, I do not consider them advantages.

Because the two measures are highly, but not perfectly correlated, it is interesting to note what difference it might have made had Benson used the more inclusive Federal Census statistics in his study of the 1844 presidential election in New York. Table 8 contains the results of regressing the percentages for each party and those not voting in 1844 on wealth, measured in three different ways. In the cases of the Whigs and Democrats, Benson-type measures show no statistically significant effect of class, operationalized in this fashion, while the

values based on the United States Census do indicate a modest effect. It is possible, therefore, that one of Benson's most celebrated conclusions about Jacksonian politics was an artifact of his choice of an index.[46]

(Insert Table 8 Here)

The side trip to New York, however, does not wholly answer Benson's criticisms of my study of Tennessee. We must therefore return south. Because whites in heavily Afro-American counties were more racist and therefore more likely to vote Democratic than whites in areas containing smaller proportions of blacks, and because white wealth correlated strongly and positively with the percentage Negro, it is difficult to disentangle race and class factors in elections in which blacks voted freely.[47] As a consequence, I attempted to determine the relation between wealth and voting among white Tennesseans during this period by choosing the election after blacks had been effectively disfranchised in which Republican support was highest and most widespread throughout the state, thereby eliminating the multicollinearity problem, but mirroring pre-1890 patterns of white voting as closely as possible. Rather than "blithely" maintaining that the 1908 results could be unthinkingly applied to the 1880s, I emphasized that many regressions, scatterplots, and contemporary newspaper reports convinced me that the pattern of Republican support in the 1908 gubernatorial contest represented "the core of white GOP strength for the 1880s," but I did not present all those materials for

obvious reasons of space. Far from trying to give the reader a false impression, as Benson asserts, I carefully explained in a footnote to the table that the class estimates were based on the 1908, rather than the 1880 or 1884 elections.[48] That the largely black Republican organizations in Middle and West Tennessee had been demolished by disfranchisement did not appear likely to affect the relation between white wealth and white voting behavior. Despite the fact that white turnout dropped substantially between 1884 and 1908, it decreased most in the relatively poor counties of East Tennessee, where Republicans were strongest, thereby diminishing, rather than increasing the relation between poverty and Republicanism.[49]

Table 9 presents one of the many sets of regressions which could be adduced to prove the continuity between white voting in 1884 and 1908. (The reader will be spared the rest of them.) The dependent variables here are the proportions of the adult male population voting for each party and not voting in 1908, and the 1884 votes are divided by the 1908 (not the 1884) population, to take account of turnover in the electorate during a generation. The black proportion is a control variable. Besides underlining the nearly total disfranchisement of blacks, the estimates give an indication of the electoral continuity among whites. Virtually all whites who voted in both elections chose the same party in 1908 as they had in 1884.[50]

(Insert Table 9 Here)

The data available to proxy the effect of class on voting in nineteenth century America is limited and imperfect. As I noted in 1974, "If subcounty differences in white wealth were consistently very large, correlations between these estimates and voting behavior would not be very meaningful." The numbers in each occupation, the proportions of farm owners, renters, and tenants, and the relationships of urban dwellers to the ownership of the means of production are not available even on the county level. Since I could determine no way to measure class consciousness, or the lack of it, in Tennessee in this period, I felt unjustified in asserting that white residents of poor counties voted Republican <u>because</u> they were poor, but only that they did "for whatever reason, end up disproportionately in the Republican camp."[51] But despite these limitations, I see no reason to withdraw from the qualified statements that I made more than a decade ago about the party preferences of white voters in comparatively rich and relatively poor counties in 1908, and I certainly have no more hesitation now than I did then in projecting the 1908 results back into the 1880s. Whatever my sins of omission or commission, Benson's jeremiad seems exaggerated, especially in the light of his own conduct.

In the last part of his "Mistransference" article, Benson histrionically retreats from his "scientistic" stance of 1957-61, disarmingly confesses that he actually arrived at his conclusions about the social bases of New York voting through "a battery of quantitative

and qualitative methods," gratuitously accuses "survey researchers and ecological regressionists" of "flagrantly" failing "to know a lot about what they are studying and . . . to think hard about what they are doing," fervently endorses a vague "methodological pluralism," and scornfully condemns methods that "require and/or exalt parsimony" in models. The confidently anti-Establishmentarian Benson of the sixties believed that "hard" evidence about voting behavior was privileged and that methods that allowed for rigorous tests and forced researchers to specify their hypotheses explicitly were to be preferred over those that did not.[52] Along with Warren Miller and Dean Burnham, Benson fostered the collection of machine-readable county-level voting data for American elections since 1824, and he spearheaded a famous summer session at Ann Arbor to proselytize for quantification and to advertise the newly-available data. The dogmatically latitudinarian Benson of the eighties, born again in the orthodox faith, denies that any type of information or technique is inherently superior to any other, denounces simplification, even if it fosters greater theoretical clarity and more certain evaluations, and disparages historians who employ methods that their peers, whom he believes to be statistical know-nothings, cannot easily replicate and assess critically.[53]

Benson's pronouncements and his practices have, as he remarks, never coincided.[54] Thus, he condemned Beard for never counterposing other hypotheses to his economic interpretation, but Benson himself presented voting returns in the Constitutional ratification election for only two extreme counties, concluding without adducing any other

evidence or attempting to assess any contrary hypothesis that they were typical of the cleavage in New York state and that they supported an urban vs. agrarian theme.[55] He lambasted Schlesinger for believing what politicians said about the bases of electoral divisions, but never in Concept simultaneously related indicators of ethnicity, religion, and class to political choice even for a single county, much less for the state as a whole.

Benson has always been more appealing for his promises than for his performances. He flung concepts at professional idols--let other, lesser scholars eventually sweep up the debris with more detailed studies. He proposed a new, plausible, general, simple, and testable theory, the ethnocultural thesis, about American voting behavior--never mind that parts of it were not wholly worked out or coherent, and that he made no comprehensive attempt to gauge its validity or to demonstrate the inaccuracy of other interpretative schemes. He touted multivariate analysis--but never actually did it. He has played the role of a creative conceptualizer and an effective salesman, but one who largely left it to others to manufacture and distribute the product. The trouble is that he now wants to declare some of his earlier opinions obiter dicta, yet to preserve the same outcomes; to eliminate some steps in his argument, yet to keep his conclusions; to discard much of what has made his work so stimulating and his influence so fruitful, and to preserve only the shell of incomplete theory and inadequate substance.

If no evidence about voting behavior has any logical precedence, why prefer Benson to Schlesinger? On what grounds, in other words, can we say that evidence from voting and socioeconomic statistics is superior to contemporaries said about who voted for whom? If all historical judgments are inextricably intertwined with historians' value-laden positions, can we and need we do any more than wear our political biases like campaign buttons on our shirts? How can we replicate an unspecifiable mixture of qualitative and quantitative evidence, which Benson says must underlie any interpretation? For instance, unless we confine ourselves to quantifiable evidence, how can we evaluate Benson's famous "suggestive" estimates (published with three decimal places!) of the patterns of party choice of New York ethnic groups?[56] Must all historians, in order to facilitate critical evaluation, bind themselves to the lowest common denominator of technical competence in the profession? How much abstraction is too much, and how much cluttered specificity is enough? If we accede to Benson's most extreme position that "all politics is local," or that patterns of human behavior are "ephemeral," won't we have to ban generalizations entirely? Is that desirable, or, unless we forswear language, which abounds with general terms, even possible?

To accept Benson's current notion that borrowing methods and criteria of evidence and arguments from the social and physical sciences and mathematics is to destroy any chance that the ethnocultural thesis or any other model of voting behavior may be proven, disproven, or replaced with a better conception. It would

force us to give up hope of ever being able to agree that one line of interpretation is better warranted than another, or even of entertaining any law-like statements. It would make replication of other scholars' work impossible. And it would deny historians any aid from the powerful theories and statistical methods that have been developed in other disciplines, which often allow more flexibility and more precise evaluations of assumptions than "commonsensical" techniques do. The mistake was not in originally importing scientific modes of thought, standards of validation, particular propositions, and methods. The tragedy now would be for historians to give them up, for it would mean regressing back into a pre-scientific, and, if Benson has his way, an anti-scientific stage.

FOOTNOTES

1. "Research Problems in American Political Historiography," in Mirra Komarovsky, ed., Common Frontiers of the Social Sciences (Glencoe, Ill.: Free Press, 1957), 113-83, 418-21, reprinted in Benson, Toward the Scientific Study of History: Selected Essays of Lee Benson (Philadelphia and other places: J.B. Lippincott Co., 1972), 3-80; Benson, Turner and Beard: American Historical Writings Reconsidered (Glencoe, Ill.: Free Press, 1960); Benson, The Concept of Jacksonian Democracy: New York as a Test Case (Princeton, N.J.: Princeton Univ. Press, 1961).

2. Concept, 331.

3. Concept, 288-89.

4. Scientific Study, 6, 8, Benson's italics.

5. Historical Methods, 17 (1984), 118-31, quotes at 118-20. Can Benson have talked to any natural or biological scientists recently? While most of them probably believe that the objects of their endeavors will ultimately be shown to be simple, the theories and data that they actually work with from day to day are usually rather disorganized and complex, and my impression is that they are generally nearly as bewildered while conducting their

research as social scientists or historians are. Benson's caricature of science as it perhaps appears in high-school textbooks serves only a polemical purpose.

6. "Mistransference," 123, 127.

7. To support his wholesale condemnation of economic theory, Benson cites one retired economist's polemic published in a Veblenite journal. That polemic, in turn, cites, in passing, only two examples, neither of which demonstrate either his or Benson's point about qualitative change. See Nicholas Goergescu-Roegen, "Methods in Economic Science," Journal of Economic Issues, 13 (1979), 317-28; and "Methods in Economic Science: A Rejoinder," in ibid., 15 (1981), 188-93. Benson offers no evidence at all for his summary dismissal of the validity of surveys.

8. "Mistransference," 122, 127.

9. "Mistransference," 121, Benson's capitalization.

10. Burnham, Critical Elections and the Mainsprings of American Politics (New York: W.W. Norton, 1970), and many of the essays in his The Current Crisis in American Politics (New York: Oxford Univ. Press, 1982). It is not clear to me that Burnham, who largely developed the ideas of V.O. Key, Jr., one of his teachers, believes that he has been reasoning analogously from any one of the numerous notions of cycle theory in economics. I find no citations to works by economic cycle theorists or to Benson's

section on voting cycles in Concept in any of Burnham's books or essays. For Burnham, in other words, there may not possibly have been any fallacy of mistransference, because there was no conscious transfer.

11. "Mistransference," 122.

12. Concept, 126.

13. The correlations for textile production were computed from figures in Lance E. Davis and H. Louis Stettler III, "The New England Textile Industry, 1825-60: Trends and Fluctuations," in Output, Employment, and Productivity in the United States After 1800 (New York: National Bureau of Economic Research, 1966), Table 4, p. 221 ("inflated" figures). I would like to thank Robert W. Fogel for bringing this and the Fishlow statistics to my attention.

14. Calculated from figures obtained from the Regional Economic Information System, BE-55, Bureau of Economic Analysis, United States Department of Commerce. The series is published in parts in the Commerce Department's Survey of Current Business, periodically. I would like to thank D. Roderick Kiewiet for providing me with these figures.

15. Alvin H. Hansen, Business Cycles and National Income (New York: W.W. Norton, 1951), 39, 50, 417-68.

16. Kousser, "Key Changes," Reviews in American History, 7 (1981), 23-28. Allan J. Lichtman's "The End of Realignment Theory? Toward a New Research Program for American Political History," Historical Methods, 15 (1982), 170-88, is the best critique of the critical elections literature.

17. Kousser, The Shaping of Southern Politics: Suffrage Restriction and the Establishment of the One-Party South, 1880-1910 (New Haven, Ct.: Yale Univ. Press, 1974), Table 5.2, p.106; Benson, "Mistransference," 124.

18. Note that the error terms do not have to be equal to zero for every observation; that is, the fit of the regression line does not have to be absolutely perfect. The behavior of states, counties, townships, or persons will always vary at least slightly from a hypothesized relationship because unmeasured variables have somewhat different effects on each individual unit. So long as these effects are not the same across all units or a large subset of them, however, the coefficients will be unbiased.

19. Benson appears to confuse regression analysis, which is a general statistical technique employed by persons in a wide variety of scientific, engineering, and other fields, with the transformation of the parameters into the form of percentage estimates of the way that members of certain groups voted. While the latter seems to be an attractive and commonsensical method of presentation, it is no more than that--a matter of style. The estimates could just as

well have been arrayed as slopes and intercepts, which, indeed, might have made clearer that the relationships of particular interest could have been "controlled" for other variables.

20. For instance, in Concept, p. 165, he comments: "Since the United States is highly heterogeneous, and has high social mobility, I assume that men tend to retain and be more influenced by their ethnic and religious group membership than by their membership in economic classes or groups." (my italics) But, of course, people do change their religions, they work with, live among, and marry persons from other ethnic groups, and they move around a great deal. Whether or not the small steps typical of social mobility in the United States--see, e.g., Stephan Thernstrom, The Other Bostonians (Cambridge: Harv. Univ. Press, 1973) and other studies cited there--make more difference to the average person's politics than the interactions among ethnic and religious groups do, and how much political difference one or another social dividing line makes at a particular time and place are surely empirical questions.

21. E.g., Michael Lewis-Beck, Applied Regression: An Introduction (Beverly Hills, Ca.: Sage, 1980).

22. Kousser, "Ecological Regression and the Analysis of Past Politics," Journal of Interdisciplinary History, 4 (1973), 237-62.

23. The italics in the text seem necessary because the point of prior discussions has apparently not sunk in. Simpler methods are not free from assumptions--in important cases the same assumptions as ordinary linear regression. Kousser, "The New Political History: A Methodological Critique," Reviews in American History, 4(1976), 1-14; Kousser, "Making Separate Equal: Integration of Black and White School Funds in Kentucky," Journal of Interdisciplinary History, 10(1980),399-428; Langbein and Lichtman, Ecological Inference (Beverly Hills, Ca.: Sage, 1978); Lichtman and Langbein, "Ecological Regression Versus Homogeneous Units: A Specification Analysis," Social Science History, 2 (1978), 172-94; Lichtman, Prejudice and the Old Politics: The Presidential Election of 1928 (Chapel Hill, N.C.: Univ. of North Carolina Press, 1979); Kousser and Lichtman, "'New Political History': Some Statistical Questions Answered," Social Science History, 7 (1983), 321-44.

24. "Mistransference," 125. Even if Benson's criticisms of my table were correct, and even if he demonstrated that in this case, I was "irresponsible," this would not logically prove the invalidity of regression, or even imply that other estimates in my book were incorrect. Naturally, the converse does not follow, either. One example, however laboriously examined, cannot by itself make or break any method. In that sense, Benson's argument is simply irrelevant to the more general question of what methods to employ to analyze electoral behavior.

25. For unexplained reasons, and in the face of clear statements and examples to the contrary in, e.g., my "Ecological Regression" article, 253-62, Benson believes that "ecological regressionists" must assume that a single relation holds for all the people in a state, and that by estimating different ones for different sections of the state I have not "remained faithful to [my] statistical techniques." He also seems to believe that I decided *a priori* to separate the data by sections because otherwise "informed historians" would have dismissed my estimates as "preposterous." "Mistransference," 125. Whatever "informed historians" would have thought, I decided to run separate sectional regressions *after*, not *before* looking at statewide regressions and scatterplots, and in the other ten ex-Confederate states, I rejected estimating regional regressions, though many regional differences seemed plausible in theory, on the same empirical, statistical basis.

26. Of course, there were other plausible models involving splitting the state into counties with low and high percentages of blacks, which improved the fit of equations in North Carolina, or adding a term for the percentage Negro squared, which led to better estimates in Georgia. In the event, neither of these models outperformed a sectional model for the Tennessee data.

27. Because the numbers of counties in any subgroup was therefore diminished, the estimates had higher standard errors than they

would have had if they had been made in one equation containing all the counties in the state. Although Benson attempts to make a good deal of this fact, which I drew attention to in my article on ecological regression, the point estimates are exactly the same as those obtained from a dummy variable approach.

28. This exposition follows that in J. Johnston, Econometric Methods, 3rd ed. (New York and other places: McGraw-Hill, 1984), 225-27. If separate regressions are run, a rather more complicated analysis of covariance must be performed to test one model against another. The dummy variable approach is much more straightforward. I want to thank Nicholas Dimsdale for bringing Johnston's admirably clear discussion to my attention.

29. Since discussion of the 1880 election would be purely repetitive, I confine myself to 1884.

30. It is of course always very difficult to determine how any small, widespread group votes, whatever methods are used. The unreliability of the regional slope coefficients for % black times East merely reflects this fact.

31. "Mistransference," 125-26.

32. "Mistransference," 124.

33. The vast majority of the Presbyterians in Tennessee were adherents to Cumberland Presbyterianism, which split from the more orthodox

Presbyterians in 1813 because of the Cumberlands' insistence on a greatly relaxed, revivalistic version of Calvinism, often imparted by ill-educated ministers. Their election not to embrace predestination prevented reunion after the Civil War. See Robert Ellis Thompson, A History of the Presbyterian Church in the United States (New York: The Christian Literature Co., 1895), 75; Ernest Trice Thompson, Presbyterianism in the South (Richmond, Virginia: John Knox Press, 1963-), I, 144-55; II, 118, on the Cumberland-orthodox battles, and II, 129-36, 261, 392-403, 446, on the social and political stance of southern Presbyterians. The familiar fact that the other southern churches in the late nineteenth century also favored state or local regulation of some, but not all social matters, but no federal regulation of race, is asserted, e.g., in John Lee Eiglimy, Churches in Cultural Captivity (Knoxville, Univeraity of Tenneasee Preas, 1972), x; Kenneth K. Bailey, Southern White Protestantism in the Twentieth Century (New York: Harper and Row, 1964), 18.

34. It is unclear what Benson means when he suggests separating "rural" from "farm" in this era in Tennessee. In any event, no statistics are available that would allow one to do so.

35. Equations 1 are merely Equations 8 from Table 2, reproduced here for purposes of comparison. Analogous equations that use Equations 7 from Table 2 as a base show similar effects. The "other church" category is left out of the equation, of course,

because otherwise the religious percentages would sum to 1.0 and multicollinearity would prevent the equation from being estimated. Multiplicative interaction terms (e.g., foreign-born times urban) produce such tiny values for all counties that they add no predictive power to the regressions and are never close to statistically significant.

36. One out of twenty times, a variable sampled from a multivariate normal distribution will pass the t test even if there was no relationship in the population from which it was drawn. Thus, one out of eighteen significant coefficients is right on target, even with meaningless data.

37. "Mistransference," 126-27. Of the three books cited in the relevant footnote on assessment practices in Benson's article, the only one available to me, Richard D. Bingham et al., The Politics of Raising State and Local Revenue (New York: Praeger, 1978), does not support the sweeping character of Benson's statement, but merely comments that late nineteenth century assessment rates varied across counties within unspecified states. A study that he did not cite, Jens Peter Jensen, Property Taxation in the United States(Chicago: Univ. of Chicago Press, 1931), referred to studies, mostly of the midwest, that showed that expensive property tended to be underassessed and inexpensive property, overassessed. This would reduce the variation, both within and between counties, and therefore would reduce, not increase the

correlation between wealth and voting. Jensen's study, that is to say, implies that if more accurate statistics had been available, I would have found a greater gap between the voting patterns of those who lived in rich and poor counties in Tennessee.

38. E.g., Sean Wilentz, Chants Democratic: New York City and the Rise of the American Working Class (New York: Oxford University Press, 1984).

39. Concept, Appendix II, 340-41.

40. In Delaware county, the only county for which he did present complete data in Concept (p.149), and where there were only sixteen towns with non-missing data, the bivariate correlation coefficient between dwelling value per family and the Democratic is -0.305. While not significant at the 0.05 level, the correlation is more suggestive than Benson makes it seem. Lacking political and social data on the town level, I could not run more extensive regressions.

41. Concept, 148.

42. Because the federal data was not available on the sub-county level, those cities (Rochester, Syracuse, Troy, etc.) could not be eliminated from the federal index. This no doubt accounts for the reduction in the correlation.

43. The sample was constructed as follows: After numbering each town and ward in a county, I chose a starting point from a table of two-digit random numbers, and, referring to the table, counted until I came to the town or ward that matched a second random two-digit number. Because Benson arbitrarily decided to exclude towns in which the census listed the value of less than 80% of the dwellings, I rejected any town that had a smaller percentage enumerated, and began the whole process again.

44. Whenever he cited specific numbers for wealth in his book, Benson apparently used data from either the 1855 or 1865 State Censuses. Since it is unclear how he incorporated the gazetteer and land value data, the only way to compare his methods against others is to employ the information from the State Censuses.

45. The means and standard deviations, aggregated on the county level, of the real and personal property per adult male (United States Census), and dwelling values per family including and excluding cities and for the town sample are as follows:

	Mean	Stand. Dev.
United States Census, 1870		
Including Large Cities	$1046.90	$499.04
Excluding Large Cities	976.11	344.48
State Census, 1865		
Including Small Cities	757.08	333.92
Excluding Small Cities	712.96	337.66
Town Sample (including small cities)	706.06	491.40
Town Sample (excluding small cities)	616.31	420.95

46. Naturally, this result can at best only be suggestive, and any serious study of the determinants of New York voting would have to take many other variables into account. It may be, however, that it is time to reopen the question of whether there was class voting in upstate New York, as Amy Bridges has done for New York City in A City in the Republic: Antebellum New York and the Origins of Machine Politics (Cambridge, Eng.: Cambridge Univ. Press, 1984), 61-102.

47. The bivariate correlation between the percentage black and the assessed real and personal property value per white adult male, allowing $100 of property for each black adult male, was 0.785 in 1884. Although Tennessee did not publish property values separated by race, some other southern states did. In North Carolina and Virginia, from 1890 to 1908, the correlations between white property values per white adult male and the estimates computed according to the same formula as for Tennessee ranged from 0.952 to 0.999. The correlation between the white wealth estimates for 1884 and 1908 in Tennessee was 0.914.

48. Shaping of Southern Politics, 104-06.

49. The regression equation, with t statistics in parenthesis, is

$$\underset{}{NV08} = \underset{(12.84)}{.302} + \underset{(9.02)}{.854\ N} + \underset{(2.41)}{.063\ E},$$

where NV08 is the percent not voting in 1908, N is the proportion Negro, and E is a dummy variable that takes the value 1 if the county is in East Tennessee, and 0 otherwise. The R^2 is 0.498.

50. As the negative estimates for cross-party voting among whites indicate, the OLS fit is somewhat unsatisfactory. Other specifications involving various combinations of regional dummies and interaction terms (% Republican times % black and % Democratic times % black) neither eliminated the negative estimates of white crossovers nor appreciably increased the explained variance. An analysis of the residuals indicated that weighting by one divided by the square root of the population would not improve the estimates. I had no access at the time that I was doing the calculations to logit or probit programs, but my past experience with those transformations suggests that the estimates of cross-party voting would have been very close to zero. The principal point of the table--that those whites who voted in both 1884 and 1908 chose the same party each time, and that the 1908 election is therefore a good proxy for white voting in the 1880s--would stand out under any specification or estimation procedure.

51. Shaping of Southern Politics, 267, 105, n.1.

52. See, e.g., "Research Problems," 7, 10, 39, 75.

53. "Mistransference," 128-30.

54. "For various reasons, I was then anxious to show that I had innovatively transferred to historical research the 'hard' quantitative methods used in the 'mature' social sciences. As a result, the methodological ideology I affirmed belied the actual practice I had followed." "Mistranaference," 128.

55. Turner and Beard, 153-7, 197.

56. Concept, 176, 185. It is not clear how to interpret Benson's claim that he meant his estimates only to indicate the "relative party support" (ibid., 167, his italics) of each ethnic group. If he intended merely to rank them, why did he not simply use an ordinal scale? If he wished to indicate that some gaps in allegiance were larger than others, why did he not simply group them into such categories as "strongly Whig," "neutral," "strongly Democratic," etc.? Regardless of what sort of scale they formed in his mind, his use of such evidence as "nuances in [New York] Tribune editorials" (ibid., 171) to decide what proportions of certain groups supported each party underlines the impossibility of replicating estimates based on "a battery of quantitative and qualitative methods."

TABLE 1

CORRELATIONS BETEEN ANNUAL CHANGES IN RAILROAD INVESTMENT BY REGIONS, 1837-1860*

	Region**				
	NE	MATL	Ohio	Far	South
MATL	.077				
Ohio	.100	.417			
Far	.000	.000	.219		
South	.104	.405	.145	.352	
SouthW	.000	.366	.382	.148	.000

* Source: Computed from data in Albert Fishlow, American Railroads and the Transformation of the Ante-Bellum Economy (Cambridge, Mass.: Harvard Univ. Press, 1965), Table 53, p.396.

** Regional Definitions:

NE = Me., N.H., Vt., Mass., R.I., Ct.
MATL = N.Y., Pa., N.J., Md., Del.
Ohio = Oh., Ind., Mi.
Far = Ill., Ia., Wi., Mo., Ca.
South = Va., W.Va., Ky., Tn., Miss., Al., Ga., Fla., N.C., S.C.
SouthW = La., Tx.

TABLE 2

STATEWIDE AND REGIONAL EQUATIONS FOR THE RELATIONSHIP

BETWEEN RACE AND VOTING IN THE 1884 TENNESSEE GUBERNATORIAL ELECTION

Panel A: Predicting the Democratic Vote

Equation #	1	2	3	4
Constant	.374(18.11)	.325(10.75)	.374(18.00)	.357(14.81)
% Black	.010(0.10)	.579(2.11)	-.006(-0.06)	.119(0.89)
% Black^2		-1.008(-2.17)		
West			.012(0.34)	.086(1.39)
% Black * W				-.309(-1.46)
R^2	.000	.054	.010	.027
Adj. R^2	.000	.031	.000	.000

Equation #	5	6	7	8
Constant	.328(16.65)	.305(14.38)	.480(21.29)	.490(21.34)
% Black	-.018(-0.22)	.117(1.23)	-.235(-2.86)	-.279(-3.28)
Middle	.123(5.50)	.196(5.33)		
% Black * M		-.406(-2.46)		
East			-.161(-6.93)	-.218(-5.48)
% Black * E				.511(1.76)
R^2	.265	.315	.364	.387
Adj. R^2	.247	.290	.348	.364

Panel B: Predicting the Republican Vote

Equation #	1	2	3	4
Constant	.382(13.82)	.475(12.10)	.381(13.71)	.426(13.65)
% Black	-.179(-1.45)	-1.251(-3.50)	-.166(-1.19)	-.471(-2.71)
% Black^2		1.909(3.17)		
West			-.010(-0.21)	-.191(-2.38)
% Black * W				.757(2.77)
R^2	.024	.129	.025	.107
Adj. R^2	.013	.108	.001	.075

Equation #	5	6	7	8
Constant	.458(19.46)	.480(18.69)	.209(8.16)	.195(7.57)
% Black	-.133(-1.39)	-.265(-2.30)	.219(2.34)	.281(2.94)
Middle	-.205(-7.66)	-.276(-6.20)		
% Black * M		.395(1.98)		
East			.262(9.90)	.344(7.67)
% Black * E				-.732(-2.24)
R^2	.425	.451	.550	.575
Adj. R^2	.412	.431	.539	.560

TABLE 2
(continued)

Panel C: Predicting Non-Voting

Equation #	1	2	3	4
Constant	.244(15.37)	.201(8.71)	.244(15.25)	.218(12.17)
% Black	.169(2.38)	.672(3.20)	.172(2.14)	.352(3.53)
% Black^2		-.901(-2.54)		
West			-.002(-0.07)	.105(2.28)
% Black W				-.448(-2.85)
R^2	.062	.128	.062	.146
Adj. R^2	.051	.108	.040	.115

Equation #	5	6	7	8
Constant	.214(13.50)	.214(12.12)	.310(16.39)	.315(16.11)
% Black	.151(2.35)	.147(1.86)	.016(.023)	-.003(-0.04)
Middle	.082(4.56)	.080(2.61)		
% Black * M		.011(0.08)		
East			-.101(-5.16)	-.126(-3.70)
% Black * E				.221(0.89)
R^2	.248	.248	.286	.295
Adj. R^2	.230	.221	.269	.270

* "t" statistics in parenthesis (t >= 2.00 is significant at 0.05 level)

** Variable definitions:
% Black = % of estimated adult male population black, 1884
% Black^2 = % black squared
West = 1 if county in western section, 0 otherwise
% Black * W = % black if county in western section, 0 otherwise
Middle = 1 if county in middle section, 0 otherwise
% Black * M = % black if county in middle section, 0 otherwise
East = 1 if county in eastern section, 0 otherwise
% Black * E = % black if county in eastern section, 0 otherwise

TABLE 3

STATEWIDE AVERAGES OF VOTING BY RACE IN THE 1884

TENNESSEE GUBERNATORIAL ELECTION

IMPLIED BY EQUATIONS 7 AND 8 OF TABLE 2

Panel A: Implied by Equations 7 (no interaction term)

Race	Voting Choice Democratic	Republican	No Vote
White	43	30	28
Black	22	47	31

Panel B: Implied by Equations 8 (including interaction term)

Race	Voting Choice Democratic	Republican	No Vote
White	42	31	27
Black	25	42	33

TABLE 4

STATEWIDE PERCENTAGES AND COUNTY-LEVEL STANDARD

DEVIATIONS FOR SELECTED ETHNIC, RELIGIOUS, DEMOGRAPHIC,

AND OCCUPATIONAL VARIABLES IN TENNESSEE, 1890

Variable	State Percentage	County Standard Deviation
Panel A: Ethnicity		
Native White, Native Parents	72.6	not available*
Foreign-Born or For. Parents	3.0	not available
Black	24.4	13.9
Panel B: Religion (Whites Only)		
Methodist	37.1	15.3
Baptist	28.3	20.3
Presbyterian	13.3	9.0
Disciples of Christ	9.2	10.3
Catholic	4.0	5.1
Episcopal	1.2	1.6
Lutheran	0.6	1.6
Other	6.2	7.5
Panel C: Urban (Both Races)		
Urban (2500+)	13.6	12.9
Panel D: Occupation (% Of Employed, Both Races)		
Agriculture	65.7	not available
Domestic Service	6.0	not available
Laborer	10.3	not available
Trade and Transportation	5.3	not available
Mining and Manufacturing	8.1	not available
Panel E: Occupation (% Of White Males >10 Years Old)		
Agriculture	68.4	not available
Professional	3.8	not available
Domestic Service	5.1	not available
Trade and Transportation	11.7	not available
Mining and Manufacturing	11.1	not available

* not available on the county level

TABLE 5

WERE THE COEFFICIENTS OF THE ORIGINAL ESTIMATES BIASED BECAUSE OF EXCLUDED VARIABLES? EXPANDED EQUATIONS FOR RACE AND VOTING IN THE 1884 TENNESSEE GUBERNATORIAL ELECTION

Panel A: Predicting the Democratic Vote

Equation #	1	2	3
Constant	.490(21.34)	.639(5.36)	.543(21.36)
% Black	-.279(-3.28)	-.248(-2.79)	-.282(-3.58)
East	-.218(-5.48)	-.164(-3.65)	-.123(-2.77)
% Black * E	.511(1.76)	.369(1.24)	.092(0.32)
Baptist		-.283(-2.22)	-.231(-3.81)
Methodist		-.153(-1.22)	
Presbyter		.042(0.26)	
Disciplea		-.140(-0.89)	
Catholic		.073(0.20)	
Lutheran		1.860(2.81)	
Episcopalian		.220(0.25)	
Brethren		-1.662(-1.27)	
Urban		-.071(-0.59)	
Foreign Born		-2.455(-1.53)	
R^2	.387	.596	.479
Adj. R^2	.364	.524	.453

Panel B: Predicting the Republican Vote

Equation #	1	2	3
Constant	.195(7.57)	.282(1.93)	.146(4.98)
% Black	.281(2.94)	.216(1.98)	.285(3.13)
East	.344(7.67)	.252(4.57)	.255(4.95)
% Black * E	-.732(-2.24)	-.527(-1.14)	-.340(-1.01)
Baptist		.085(0.55)	.216(3.09)
Methodist		-.046(-0.30)	
Presbyterian		-.303(-1.54)	
Diaciples		-.134(-0.70)	
Catholic		.211(0.47)	
Lutheran		-1.795(-2.21)	
Episcopalian		.235(0.21)	
Brethren		2.563(1.60)	
Urban		.151(1.03)	
Foreign Born		-2.276(-1.16)	
R^2	.575	.669	.620
Adj. R^2	.560	.610	.601

TABLE 5
(continued)

Panel C: Predicting Nonvoting

Equation #	1	2	3
Constant	.315(16.11)	.079(0.70)	.311(13.28)
% Black	-.003(-0.04)	.033(0.39)	-.003(-0.04)
East	-.126(-3.70)	-.088(-2.07)	-.132(-3.20)
% Black * E	.221(0.89)	.158(0.56)	.248(0.92)
Baptist		.198(1.65)	.015(0.26)
Methodist		.200(1.62)	
Presbyterian		.261(1.72)	
Disciples		.274(1.85)	
Catholic		-.284(-0.82)	
Lutheran		-.065(-0.10)	
Episcopalian		-.455(-0.54)	
Brethren		-.901(-0.73)	
Urban		-.080(-0.70)	
Foreign Born		4.731(3.13)	
R^2	.295	.430	.296
Adj. R^2	.270	.329	.261

TABLE 6

THE EFFECTS OF ADDING SIX MEANINGLESS VARIABLES

TO EQUATIONS 8 FROM TABLE 2*

Ind. Var.	Dem.	Rep.	No Vote
Constant	.512(10.77)	.152(2.76)	.336(7.97)
% Black	-.311(-3.65)	.285(2.89)	.025(0.34)
East	-.198(-4.90)	.330(7.06)	-.133(-3.71)
% Black * E	.307(1.05)	-.579(-1.71)	.272(1.05)
Garb. #1	.076(2.13)	-.018(-0.44)	-.058(-1.83)
Garb. #2	-.053(-1.47)	.064(1.53)	-.011(-0.34)
Garb. #3	-.022(-0.66)	.001(0.01)	.022(0.73)
Garb. #4	-.006(-0.19)	.008(0.22)	-.002(-0.07)
Garb. #5	-.048(-1.27)	.054(1.23)	-.006(-0.17)
Garb. #6	.010(0.29)	-.019(-0.46)	-.009(0.28)
R^2	.465	.606	.333
Adj. R^2	.402	.560	.255

* Garb. #1 - Garb. #6 are columns of random numbers generated by a computer program. The other variables are from Equations 8 in Table 2.

TABLE 7

STATEWIDE AVERAGES OF BLACK AND WHITE VOTING IMPLIED BY

ADDING "BAPTIST EFFECT" TO EQUATIONS 7 AND 8 OF TABLE 2*

Panel A: Equations 7 + "Baptist Effect"

	Vote Choice		
Race	Democratic	Republican	No Vote
White	43	29	28
Black	18	51	31

Panel B: Equations 8 + "Baptist Effect"

	Vote Choice		
Race	Democratic	Republican	No Vote
White	43	30	27
Black	18	49	33

* % Baptist set at its statewide mean, aggregated by counties

TABLE 8

CLASS VOTING IN THE PRESIDENTIAL ELECTION OF 1844 REVISITED:

DID BENSON'S INDEX PRODUCE HIS RESULTS?

Panel A: Predicting the Democratic Vote*

Equation #	1	2	3
Constant	.502(21.84)	.453(22.25)	.451(32.05)
Property	-.051(-2.30)		
Dwellings 1		-.002(-0.08)	
Dwellings 2			.002(0.10)
R^2	.093	.000	.000

Panel B: Predicting the Whig Vote

Constant	.380(16.74)	.430(21.28)	.429(30.77)
Property	.052(2.39)		
Dwellings 1		.001(0.06)	
Dwellings 2			.003(0.18)
R^2	.099	.000	.001

Panel C: Predicting the Liberty Vote

Constant	.042(3.59)	.061(6.80)	.045(6.98)
Property	-.009(-0.80)		
Dwellings 1		-.036(-3.39)	
Dwellings 2			-.017(-2.32)
R^2	.012	.181	.094

Panel D: Predicting Nonvoting

Constant	.077(4.26)	.056(3.82)	.075(7.20)
Property	.008(0.44)		
Dwellings 1		.037(2.09)	
Dwellings 2			.013(1.07)
R^2	.004	.078	.022

* The denominator for party percentages and nonvoting is the number of voters in 1845, as printed in the 1865 New York state census. The independent variables are: property = the real and personal property value from the 1870 U.S. census, divided by the number of adult males, from the same source, measured at the county level; dwellings 1 = the value of dwellings per family, taken from the 1865 New York state census, also measured by county; dwellings 2 = dwelling values per family for a random sample of the towns and wards in each county in 1865. The coefficients for the wealth variables are stated for every $1000 of wealth.

TABLE 9

THE RELATION BETWEEN VOTING PATTERNS IN THE 1884 AND 1908

TENNESSEE GUBERNATORIAL ELECTIONS, CONTROLLING FOR RACE

Panel A: Regression Coefficients for 1908 Voting & Nonvoting

Vote in 1908 for	Dem.	Rep.	No Vote
Constant	.180(6.54)	.313(6.42)	.504(8.09)
% Black	-.008(-0.20)	-.785(-10.76)	.804(8.64)
Dem. '84	.590(11.03)	-.399(-4.21)	-.184(-1.51)
Rep. '84	-.310(-5.56)	.623(6.30)	-.302(-2.39)
No Vote '84	-.010(0.13)	.190(1.30)	-.223(-1.20)
R^2	.785	.736	.508
Adj. R^2	.775	.723	.484

Panel B: Estimates of Continuity Among Whites Implied by Panel A

Vote in 1908 for	Dem.	Rep.	No Vote
Net Entrants	.18	.31	.50
Dem. '84	.77	-.09	.32
Rep. '84	-.13	.94	.20
No Vote '84	.19	.50	.28